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Department of Bacteriology.

20th June, 1952.

Dear Lederberg,

I must apologise for not having written to you for such a long time but I have been very busy during the last six weeks. Many thanks for the non-lysogenic W-1982. I have not tried it yet but I did obtain an F+ non-lysogenic, histidine and cystine-requiring strain from Lwoff and this responds to UV in exactly the same way as 58/F+ when mated with W-677. The fertility enhancement by UV is therefore a primary effect. Thanks, too, for the reprint on replica plating which I had intended writing to you for. I think this direct proof of the spontaneity of bacterial "mutations" is one of the neatest things which has come out for a long time. As regards the "streptomycinase", I spoke to Lightbown as promised but learned that he had already written to you directly about it and, since I was busy, I did not bother to write.

I have done a few more things about recombination. The first is that, in F+ X F+ mates, the effect of UV on either of the F+ strains is to increase its "F value" (as you would say). The recombination rate rises and the distribution of marker characters (Lac, Mann, Mal, Gal, SM) of prototrophs shifts markedly towards the phenotype of the unirradiated strain which therefore behaves as if it was relatively F-. If both strains are irradiated, the balance of phenotype distribution is restored and the recombination rate is greater than if either mutant alone is radiated. This, I think, is against your idea that mating efficiency is a function of the F value differential between the two partners since, if this was so, then equalising the F value of both strains by treating both with UV should result in a drop in recombination rate as against that shown when either strain alone is irradiated. The second thing is that I have produced evidence against my theory (which I have no doubt you regarded as amateurish, naive and altogether unworthy of consideration !) that

/over

F+ and the mechanism of gene transfer might be identical. UV treatment of the F+ strain appears to diminish F+ transfer while increasing recombination rate. Moreover, SM treatment appears to lower the rate of F+ transfer much more than recombination rate. I have only done one experiment but the results seem clear enough. Here they are:

The mate was 58-161/F+ X W-677/S^r/F- and the duration of exposure of the F- strain to the F+, treated in various ways, was 2 hours in nutrient broth. 25 colonies of W-677 were isolated and tested for F+ conversion from each mixture and, of course, precisely the same technique was used for each.

F+ culture treated:	No. prototrophs.	Percent F+ conversion.
Nil	122	56
UV	640	32
UV + SM (count reduced 10^{-4})	495	4

In an experiment just cooking I seem to have got 2/50 isolates of W-677/F-, grown overnight in a collodion filtrate (0'74μ APD) of a young broth culture of 58-161/F+, which have been converted to F+. An equal volume of filtrate to that in which W-677 was grown was sterile on incubation and no Lac+ colonies were isolated from a plating of the test filtrate for W-677 isolation.

I will have to check up on the converted isolates of course, and do the whole thing again.

All this makes writing a discussion for a paper very difficult ! My own feeling is that the process of recombination is probably unique and, as you wrote me on one occasion, that one must be guided only by the facts as they come to light. I still think that there is a very good case for one-way gene transfer, however.

Please let me know if you have any good ideas.

With best wishes,

William Hayes

P.S. I am sending a copy of this letter to Cavalli for his information.

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